Review of

Isoneutral control of effective diapycnal mixing in numerical ocean models with neutral rotated diffusion tensors
[Hochet, Tailleux, Ferreira, Kuhlbrodt]

One cannot define a density variable that is both a material surface, and along which parcel exchanges are energetically neutral. Thus ocean models that rotate the diffusion tensor (a la Redi 82) to a neutral frame, in order to separate large isopycnal from much smaller diapycnal diffusivities, will incur inadvertent diapycnal diffusion due to the isopycnal part. This paper seeks to make this clear, and compare the relative spurious mixing errors incurred by the use of various neutral density variables. The analysis is done not with an ocean model, however, but with hydrography. The results are that it doesn't matter too much, except in about 5% of the locations.

My chief concern is that, as implied above, while this paper seems to be directed toward ocean models, no attempt is made to connect their results to what is actually done in models, beyond the idea of the rotated Redi diffusion tensor. What neutral vectors do MOM, NEMO and the MITgcm use, for example? How would the present results depend on model resolution? Are the implied errors significant relative to other errors, or are they negligible? I very sincerely have no idea what to do with the information the authors have given me, or whether it is something a real modeler needs to worry about, or not.

This confusion allows for some lofty and silly claims in the conclusion, especially the last two sentences of the paper, where physical ideas are conflated with a modeling issue.

Specific points

P 3, L18-28: This argument is indecipherable. What is \hat\rho? Where does (7) come from? (the authors say "The expression for the neutral vector *becomes*…" 'becomes' from what? And there is an error below (8): that J must be \partial\gamma/\partial S, not the Jacobian term, which is already in (8).

For reference, I have read the relevant parts of Tailleux (2016b), which is well-written and careful. In the present paper, the authors are sloppy and assume too much of the reader.

P 5, L 17: Reference Gent and McWilliams 90 and at least briefly explain v_gm!

Sec. 2.2 & Appendix B: Conversion to density (or any other tracer) coordinates has been done over and over in the literature. Admittedly, it's a messy literature, but it doesn't need to be repeated here. In particular, there is only one effective diffusivity. I haven't read Speer 97, but I've read WD96, Nakamura 96 and Shuckburgh and Haynes 03 (SH03) in great detail — the latter is the most useful. There is an error in the effective diffusivity formulation of WD96 that stems from their incorrect eq. (9), and it

seems to have made its way to this paper in (18), though I am not certain because I'm a little uncertain about \nabla z_r.   I would in any case recommend the authors look at SH03, equations (6) & (7) (and compare to WD96 eq. (12)). That said, I doubt it will affect the results much.

Regarding Speer's Keff, is (20) correct? It would say Keff = K if K is constant… surely that's not right.

Sec 3: I am very surprised that none of the reviewers asked for this: please add a detailed description of the algorithm used to compute your Keff. Given the errors and mess in the literature, it's essential.

Throughout: There are misspellings and incorrect uses of plurals and s endings. e.g. "…the small number of pointS with …" and "… could correlates …" and "This is at oddS with …"